Dear Dr. Shemesh,

Please find our reply to the reviewer's comments below. We are grateful for the thorough comments and are convinced that we have addressed all points satisfactorily.

Best wishes

Gerald Langer (on behalf of all authors)

Detailed reply to the reviewer's comments:

Reply to Y. Dauphin:

REPLY: We would like to thank Y. Dauphin for this thorough comment and are confident that we have satisfactorily addressed all the points below.

Nevertheless, some questions persist.

First, only some specialists are able to identify

the species of Patella. Only small details are important, and because even the shells of living animals are eroded or encrusted, color patterns are not well visible. Several species often co-exist in a single site. Thus, the taxonomy of "Patella" is still controversial. The morphology (inner and outer views) of the samples will be useful.

REPLY: Patella samples were checked by Francesco Paolo Patti (SZN, Ischia) who confirmed the taxonomy. We included photographs of the shells we analysed in the revised manuscript (Figure S1).

2nd:

microstructural observations are missing. The structure of the shell of this genus is unique: the crossed lamellar layer (the most common structure in Molluks) is calcitic.

Some sublayers are aragonitic. But the main part of aragonite is a prismatic layer related to the muscle insertion (myostracum). The absence of thin sections or SEM pictures does not allow the reader to understand what is the structure of what is called "aragonite" in the manuscript.

REPLY: While we agree that it would be, per se, interesting to have information on the microstructure of the shells, this information is irrelevant with respect to the scope of the present manuscript. For our purpose it is sufficient to know which parts of the shells are aragonitic and which are calcitic. We are looking forward to addressing the question of microstructure in the future, though.

Third: despite the high quality of Raman analyses, other cheaper and faster techniques are available: BSE SEM images with or without staining (Feulgen, spatial resolution about 1 micron), staining of thin sections.

REPLY: Since several techniques were available, we had to make a choice. We did choose Raman microscopy.

Fourth: we have no data about the age of the samples, and the duration of their exposure to the acidic site.

REPLY: The samples were exposed to the respective sites (normal versus low pH) all their lives. The exact age of the samples is unknown to us. They are probably several years old.

Fifth: there is no data about the parameters of the sea water (salinity, temperature, agitation...); these parameters play a role in the life of the animal, as the nutriment does.

Some decades ago, it was shown that the calcitic/aragonitic ratio in Mytilus shell de-

pends on the sea water salinity (Dodd in the 60's). This controversial interpretation was not confirmed and it has been shown that other factors play a role. It seems there is a similar situation here. The authors of the manuscript deal with a topic regularly mentioned in all the past and present, national and international projects in which the future climate is concerned. They do not compare their results to what is described for other molluscs. At last, the observations are not sufficient enough to be so affirmative regarding the conclusions. The authors must discuss other hypotheses.

REPLY: We agree insofar that data based on field samples do not warrant the same degree of interpretative certainty as data based on experimental samples. Having said that, the field samples we used are very special and probably as close as one can get to a laboratory experiment. The reason for this is that the Ischia CO2 vent site features a classical DIC-manipulation scenario (for details see e.g. Hoppe et al. 2011) without disturbing secondary influences. We have added a paragraph dealing with possible secondary influences:
"We ascribed the changes in shell mineralogy and shell thickness of our samples to seawater carbonate chemistry changes. Since these are field samples, as opposed to experimental samples, possible secondary influences have to be considered. For Mytilus it was shown that maybe salinity, but certainly temperature influences the aragonite/calcite ratio (Dodd 1966, Eisma 1966). The latter is also true for Patella (Cohen and Branch 1992). In our case, however, both temperature and salinity at the two sites (Figure 1) were the same at any given time (Table 1, Cigliano et al. 2010, Hall-Spencer et al. 2008, Rodolfo-Metalpa et al. 2011), and we conclude that these two parameters did not influence the aragonite/calcite ratio of our samples. Also both the control site and the low pH site we sampled at Ischia are sheltered so that there is no difference in wave action, which could potentially influence shell architecture. Furthermore, it was suggested that the concentrations of inorganic ions such as Mg and Sr can influence the mineralogy of marine calcifying organisms (Watabe 1974). Since salinity was constant in our case, the concentrations of major ions such as Mg and Sr were likewise, and their influence can be ruled out. On the other hand, shells from the low pH site clearly are corroded (see above), so there is a massive impact of seawater carbonate chemistry on the organism. Taken together with the constancy of other environmental parameters, that leads us to conclude that carbonate chemistry changes are the best explanation for the changes in shell mineralogy and shell thickness of our samples."
The effect of the corrosive water at the low pH site is further illustrated by a number of images at our disposal. We attach a few examples to this reply, but don't think it is necessary to include them in the manuscript, because the corrosive nature of the low pH site is already well documented (see page 12573, line 23 of our manuscript).

Figure Caption:
Fig. 1 Gastropod molluscs living adjacent to shallow water CO_2 seeps off Ischia showing severely eroded shells due to the corrosive effects of the seawater a) Hexaplex trunculus, b) Osilinus turbinata and c) Patella

caerulea. At control sites these gastropod species were common but were never found with dissolved shells like these

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End of Reply to C4853Interactive comment on Biogeosciences Discuss., 11, 12571, 2014.

Reply to referee 2:

Reply: We would like to thank Referee 2 for this constructive comment. We have addressed all the points below.

Remarks:

- p2 l.10-12: the formulation is quite intriguing : the calcite layers still keep growing in thickness. (or not ?) cf. comments below.

Reply: We cannot be sure. It appears to us that the calcite layers might indeed keep growing in thickness, but only during elongation growth (i.e. "normal growth") as opposed to what we have called "enhanced" or "compensatory" shell production. Since we cannot know whether the material produced during compensatory shell production represents growth layers or structural layers, we substituted "parts" for "layers" in the abstract. That makes clear that there is a vagueness here.

- p8 l.11: " inside " (center of the shell) or " inner side " (the whole growth surface of the shell) ?

Reply: The "inner" side. We changed the word. It is not the whole growth surface, however, but only the aragonitic parts, as we described in the following sentences.

- p8 l.21-23: " This mechanism allows for compensatory shell thickening through the deposition of additional layers on the inside of the shell ".

Not clear to me. What does " layer " mean here ? Growth layers or structural layers ? Patella shells can display up to 7 structural layers, displaying crossed lamellar (XL) (aragonite), cross-foliated (CF) (calcite) or myostracal (M) microstructures (McClintock, 1967). They are all deposited synchronously, at each growth increment (" growth layer"), on the inner surface of the shell. Does the authors mean thicker (and not " additional ") growth layers in the center of the shell than in the border (therefore, just different calcification rates in the two zones) ? Or is there a specific deposit (additional " structural " layers) that recovers the center of the shell, in a mechanism that could be more related to shell -remobilization or –repair processes ? These latter are indeed quite frequently observed in gastropod shells (and display specific microstructures, ex. Fleury et al, 2008). It is hard to decipher without a microstructural investigation, that would be much welcomed to validate the mechanism proposed by the authors. The absence of such aninvestigation is intriguing, as some features are already visible in the Confocal Raman Microscopy pictures provided (in Fig 4 : growth lines, cross-foliated

lamellae in M+2/M+3 layers, etc.). Why not provide some more resolute maps ? It seems like then present manuscript acts like a preliminary study, meaning to precede a more complete microstructural investigation. It have no objection to it, given it is clearly stated in the manuscript (in the conclusion perhaps).

Reply: As referee 2 correctly says, we cannot know this without a microstructural investigation, which is indeed a follow up study. We did as suggested by referee 2 and stated that in the conclusion. We also added the following to clarify:

"We do not know whether the additional layers are structural layers. One possibility is that the layers we call "additional" are similar to the layers related to shell repair in *Haliotis* (Fleury et al. 2008)."

- p9: actually, the conclusion is just a copy/paste of p6l20-25, making it redundant and

not very useful.

Reply: We modified the Conclusion. It now reads:

"Polymorph distribution analyses of complete cross sections of *Patella caerulea* shells from a CO_2 vent site at Ischia revealed that this species counteracts shell dissolution in corrosive waters by enhanced production of aragonitic shell layers. The question whether these layers represent structural layers will be the subject matter of an upcoming microstructural investigation."

- I am not native english, but the spelling and syntax seem fine to me.

Interactive comment on Biogeosciences Discuss., 11, 12571, 2014.